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VII.

NOTE ON A CRITICISM OF THE AUTHOR'S APPARATUS FOR FRACTIONAL CONDENSATION.*

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The supplement of the American edition of the Chemical News for February, 1869, contains an article on "Cracking of Petroleums," by the editor of the supplement, which closes with the following paragraph: —

"In conclusion, we offer the suggestion to those who analyze oils by fractional distillation that they take pains to avoid the conditions which favor cracking; by not observing such caution the analyses of petroleums hitherto made have generally been vitiated, in fact, very many have been worthless. Perhaps it is not possible to avoid all the error that may arise from cracking, yet it may easily be brought so low as to be insignificant. Of the apparatus which has been devised for fractional distillation probably the one least suitable for petroleums and other unstable liquids is that of Prof. C. M. Warren. Warren's still probably answers well enough for alcohols, essential oils, and the benzole series, but cracking can scarcely be avoided when it is used for petroleums."

I am induced to notice the above criticism from the fact, to which it recalls my attention, that the question of adaptability for the separation of bodies more or less liable to decomposition by heat in distillation, as compared with that of the common forms of distillatory apparatus, was not specially considered in my memoir descriptive of my process,† an omission which, it now appears, it might have been better not to have made. It was not, however, from oversight that this was omitted, but because it was thought that no one could fail to see, on a moment's reflection, that the conditions to which liquids are subjected in my apparatus, so far as these may be of a nature to affect the liability to decomposition, are quite the same as those attending fractional distillation by the common process. It was therefore deemed sufficient to make no further allusion to this matter than was done, indirectly, in recommending my process for the separation of *"bodies not decomposed by heat in distillation"* (p. 134 of the original memoir), clearly implying that I did not regard it as faultless for the opposite class of substances; and sufficient, it would seem, to put any one on his guard who would make such use of it. Nevertheless, as I hold that my process introduces no new condition favorable to decomposition, even of bodies liable to partial change of this nature during distillation, I do not hesitate to recommend it also as preferable to the old process for this very class of substances, — when distillation cannot be altogether avoided, — since the length of time required to effect an equal or better separation may thereby be very much shortened, and consequently the aggregate amount of decomposition materially and proportionately lessened. But in operating upon such substances it would be unadvisable to continue the process longer than absolutely necessary to obtain the best approximation to the desired result which the nature of the material would admit of; and the same is of course true of the old process.

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* It would appear that this article was written in 1869. Two manuscript copies of it were found among the author's papers after his death, viz. a rough draft, and a fair copy ready for the printer's hands. The latter is here printed, without change.

† Memoirs of the American Academy, 1864; American Journal of Science, 1865; Chemical News, 1865.

Not to enter into much detail, it may be stated that the only essential difference between my apparatus and that in common use for fractional distillation consists in the elevated condenser which I interpose between the retort and the ordinary condenser, discharging itself backward into the retort, its temperature being regulated by a separate flame, and maintained a few degrees (more or less, according to the nature or purity of the substance to be distilled) below the temperature of the retort.

In other forms of apparatus, there being no regulated condenser to effect a separation of the mixed vapors that escape from the retort, the whole of these go forward and are condensed together, the resulting liquid falling into the receiver.

Both in my process and in the common process the liquid in the retort is simply maintained in a state of ebullition under atmospheric pressure; consequently it is no more favorably conditioned for decomposition, in respect to temperature, in the one case than in the other.

The elevated condenser being kept at a lower temperature than that of the retort, it seems needless to say, has no effect to raise the temperature of the latter, but, on the contrary, holds it in check; and by proper management may serve to secure the conditions most favorable for taking off the largest possible quantity of the most volatile constituent at any time present, and this at the lowest possible temperature, not only of the vapor to be collected, but also of the liquid in the retort, — operating therefore advantageously, rather than unfavorably, with respect to decomposition. And yet I suspect, as he does not specify, that this is the identical feature of my process — being its chief or only distinctive feature — which has occasioned my critic's misconception with regard to the utility of the apparatus. It would evidently require a considerably longer time to distil a given quantity of a mixture of liquids of different boiling-points in my apparatus, if used as recommended for stable liquids, than in the common form, since in a single operation the larger part of the mixture is in reality many times condensed and re-distilled. Can this be his stumbling-block, — that he has not taken into consideration the relative amount of actual work (not measurable, however, by the *quantity* of liquid distilled, but by the *degree of purity* of the products obtained) that may be done with this apparatus in a given time as compared with the common apparatus?

If the liquid is not subjected to a higher temperature in the one apparatus than in the other, it is evident that superiority on the score of decomposition must be awarded to the one capable of giving the best results in the shortest space of time, since the amount of decomposition must be proportional to the time occupied. If objection is made to the repeated condensation and re-distillation that take place in my apparatus, I have only to say that this is a necessary condition of any effective process of fractional distillation; and if, to avoid cracking, this condition must be abandoned, and the vapors, as they rise laden with impurities, are immediately condensed in the receiver, then must fractional distillation itself be abandoned as a worthless process.

It has probably escaped my critic's scrutiny that my apparatus does not require to be employed in precisely the manner that I recommended for stable liquids; and a judicious chemist would modify the conditions to suit the nature of the material to be examined, — I refer to the adjustment of the temperature of the elevated condenser. In operating on perfectly stable liquids, I have found it advisable to keep the elevated condenser at as low a temperature as would permit the vapor of the most volatile liquid to pass through it; for in proportion as its temperature is higher than this, the product will contain a larger quantity of the less volatile constituents; and if the temperature be allowed to equal that of the retort, the conditions of the experiment would not differ essentially from ordinary distillation, except that friction, from contact of the vapor with the sides of the worm during its passage through the elevated condenser, might give it some slight advantage. Between these two extremes of temperature for the elevated condenser, therefore, it may be adjusted suitably for any unstable liquid requiring and admitting purification by distillation, since the time required to distil a given quantity of liquid by this process may be shortened, other things equal, in proportion as the temperature of the elevated condenser is raised nearer to that of the retort; and by shortening the time, other conditions the same, decomposition, as said before, is proportionately lessened.

I might give the details of many experiments to prove the superiority above claimed for my apparatus, for it has given me reliable results under the most varied conditions; but this does not seem to be required. I trust that the foregoing remarks will be sufficient to lead any one who may have questioned the adaptability of my apparatus for the class of substances now under discussion to view it in a different light.